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## DISCUSSION AND CORRESPONDENCE

### ALGONKIN LANGUAGES OF CALIFORNIA: A REPLY

IN a paper on "Wiyot and Yurok, Algonkin Languages of California," published in this journal (N. S., vol. 15, 1913, pp. 617-46), I ventured, on the basis of lexical, morphological, and phonological evidence, to demonstrate that Wiyot and Yurok were very divergent outlying members of the great Algonkin linguistic stock. No one is more keenly alive than myself to the probability that not a few of the lexical and morphological elements compared in this article may, on maturer knowledge, turn out to be untenable parallels, but it seemed and still seems to me that the general cumulative evidence presented is so strong and that many of the specific elements compared are so startlingly similar that no reasonable doubt could be entertained of the validity of the claim. However, Dr Michelson is evidently far from convinced, for in a note entitled "Two Alleged Algonquian Languages of California," also published in this journal (N. S., vol. 16, 1914, pp. 361-67), he adopts a severely critical attitude and tries to show, apparently to his entire satisfaction, that Wiyot and Yurok are not and can not (we might almost add must not) be Algonkin. Five main reasons are given for his dissent, followed by a list of seventeen specific respects in which Wiyot differs from Algonkin, and of eight in which Yurok so differs. Considerable space is then taken up with the attempt to prove that five or six morphological elements have been wrongly compared (leaving the reader to conclude, I presume, that all other comparisons of elements suffer from the same defect), after which some rather random data are given to show that accidental linguistic resemblances crop up in all sorts of places.

Dr Michelson triumphantly ends: "Enough has been said to show the utter folly of haphazard comparisons unless we have a thorough knowledge of the morphological structure of the languages concerned. It is for this reason that I have refrained from endeavoring to compile a list of fancied lexicographical resemblances between Wiyot and Yurok with other languages than Algonquian ones, and a list of such similarities between Algonquian and other languages than Wiyot and Yurok." It is difficult for one convicted of "utter folly" to gain a hearing in

self-defence, for he is to be trusted no further. And yet I shall try to rescue at least part of the painfully constructed edifice that Dr Michelson has sent tottering over my head and to show that the trouble with Wiyot and Yurok, like the premature report of Mark Twain's death, is "greatly exaggerated." As for Dr Michelson's self-denial exhibited in the passage quoted, it can but elicit our admiration, the more so as he has in this way escaped from what would no doubt have proved an onerous task. Still I somewhat regret that Dr Michelson did "refrain," for as goodly an array of say Fox-Chinese parallels of equal inherent plausibility and consistency as those I have given for Algonkin and Wiyot-Yurok would have done more to convince me, for one, of the absurdity of my thesis than anything else in his reply.

Among the five main "reasons" for his refusal to follow me in my argument I am concerned to find that Dr Michelson lists: "that fancied <sup>1</sup> lexicographical similarities have little or no weight in view of the above [four] points." It would seem, if I understand Dr Michelson rightly, that my case would have been stronger, if anything, if I had left out most of the approximately two hundred lexical correspondences that I give; at any rate, only four points could then have been scored against me. But what is one to do if the bulk of his evidence is summarily ruled out of court on considerations of a largely irrelevant character? A little later on we are told that "the apparently abundant lexicographical material does not impress" Dr Michelson. All I can say is that if one is not "impressed" by such truly remarkable parallels as Yurok *n*- "my," *q*- "thy," *o*- "his," *m*- "somebody's" (*with body-part nouns*): Algonkin *n*- "my," *k*- "thy," *o*- "his," *m*- "somebody's" (*with body-part nouns*);<sup>2</sup> by Wiyot *m-èpt* "tooth": Cree *m-ipit* "tooth"; Wiyot *māti* "wood": Cree *misti-k*, Cheyenne *mata*; Wiyot *mākw* "grizzly bear": Cree *maskwa* "bear"; Wiyot *mīw*- "to eat (slain animal)": Cheyenne *miv* "to eat (animate object)"; and many others, his individual psychology differs markedly from my own. There is no accounting for tastes,

<sup>1</sup> Would it not have been wiser to leave this word out? It savors of prejudgment, I cannot help thinking.

<sup>2</sup> These four pronominal prefixes must be valued as a unit, instead of being bandied about, as is done by Dr Michelson, torn out of their setting. The evidential value of an *orderly sequence*  $a + b + c + d$ :  $a_1 + b_1 + c_1 + d_1$  is of course vastly greater than of  $a : a_1$  or  $b : b_1$  singly. Of the logic of cumulative and associated evidence Dr Michelson seems to have hardly an inkling. It is worthy of note that on the most remarkable of these four possessive prefixes, *m*- "somebody's," Dr Michelson is discreetly silent. In general, the really convincing *positive* points raised in my paper are sedulously avoided in his reply.

but it seems to me that such callousness deserves to be called linguistic cynicism almost as much as scientific sobriety.

Another one of the five dissenting "reasons" given is: "that many of the supposed similarities in morphological elements must be considered as accidental, for they occur likewise in a number of other languages." The "for" of this statement seems to me to involve a dangerous dogma. Does Dr Michelson seriously maintain that the homology of features  $a, b, c, d, \dots$  of complex  $A$  to features  $a_1, b_1, c_1, d_1, \dots$  of complex  $A_1$  is rendered spurious by the fishing up of such further straggling homologies as of  $a$  to  $a_2$  of complex  $A_2$ ,  $b$  to  $b_3$  of complex  $A_3$ ,  $c$  to  $c_4$  of complex  $A_4$ , and so on? I can see neither logic nor mathematics in his thesis. One might apply it with disastrous results. Thus, the existence of three grammatical genders (masculine, feminine, and neuter) in both Latin and Greek is irrelevant to the question of their genetic relationship, "for" Chinookan also possesses this feature! Further, the presence of a case system in Latin and Greek is of no account in view of the fact that Finnish also possesses one! Or, to make use of a specific morphological element, it is hardly worth while comparing feminine noun-ending  $-a$  of Latin with  $-\alpha$  of Greek when we remember that prefixed  $a$ - occurs as characteristic of feminine nouns in Wishram! Evidently Dr Michelson overreaches himself here. Moreover, at the very best, Dr Michelson's "reason" merely refuses to meet one problem because another incidentally presents itself.

The greatest stress, however, seems to be laid by Dr Michelson on his first "reason": "that the published Wiyot and Yurok material indicates that both have many morphological traits which are thoroughly un-Algonquian." The seventeen articles debarring Wiyot from membership in the Algonkin family are given in support of this statement. Here the two most serious flaws in Dr Michelson's whole standpoint come out glaringly enough. In the first place, his remarks indicate a startling lack of perspective in linguistic comparison; in the second place, he makes an altogether illegitimate use of negative evidence, in a few cases, curiously enough, even turning a positive argument of mine into a negative argument to the contrary.

Taking up the first of these flaws, we must bear in mind that the greater the historic chasm separating languages of common origin, the more profound may be expected to be the divergences that present themselves. We do not expect Swedish and German to show as many and as striking similarities as a Saxon German dialect and a Swabian German dialect; nor German and Latin as many and as striking similarities as

German and Swedish; nor German and Bengali as many and as striking similarities as German and Latin. Turning to America, the parallax of linguistic history absolutely demands that Wiyot and Yurok, granted their relationship to Algonkin, present vastly greater differences from those languages that are generally classed as Algonkin than the extremes of these (say Cheyenne and Micmac) present among themselves. Indeed, from the viewpoint of Wiyot, all the Algonkin languages now recognized as such have not improbably to be considered as a single language exhibiting relatively trivial dialectic divergences, just as the differentiation of Slavic languages today is a quite secondary phenomenon when viewed from the angle of the relationship of say Celtic and Indo-Iranian to Slavic. It is thus obvious that if by Algonkin is meant what Dr Michelson means by it, Wiyot and Yurok *must* "have many traits which are thoroughly un-Algonquian." Russian cannot be more Germanic than it is Indo-Germanic; the residue of Slavic minus Germanic, as it were, which is contained in Russian is, of course, as "thoroughly" non-Germanic as you like.

If, now, we turn to Dr Michelson's list of divergences, we are almost thunderstruck by the triviality of many of them; we are even inclined to feel hurt that Dr Michelson should for a moment have wanted Wiyot to be so Fox-like. Thus, we read: "A demonstrative element *ru-* is [in Wiyot] frequently prefixed to verbs, [while there is no such prefix in Algonkin]." One can only shrug his shoulders and ask a puzzled "Well?" Or: "A special particle is [in Wiyot] always attached to the first word of an interrogative sentence, [but not in Algonkin]." This point of difference need not unsettle us, when we remind ourselves that, e.g., while Latin has an enclitic interrogative particle *-ne*, its lineal descendant French has no such thing. Even more instructive as throwing light on Dr Michelson's sense of perspective is this item: "The stem-vowel of a verb is not changed [in Wiyot] to form a participial [as it is in Algonkin]." As a matter of fact, while internal vocalic change of the type referred to is found well developed in, e.g., Cree, Ojibwa, and Fox, it has not, so far as I know, been discovered in such undoubtedly Algonkin languages as Micmac, Natick, Blackfoot, or Cheyenne; in other words, the feature may turn out to be rather a special development of a group of Algonkin languages than characteristic of Algonkin as such. Would Dr Michelson expect Old Irish to resemble a Prakrit dialect A more than said dialect resembles Prakrit dialect B? It is not necessary to refute Dr Michelson's items one by one, my aim being rather to point out the general spirit of the criticism. They are either of the trivial nature already illustrated,

rest on incomplete analysis of the facts, or, at best, can not be justly held to outweigh in a problem of this kind the mass of positive morphologic evidence I have given. Curiously enough, some of the shots fired are merely blanks. In no. 2 we read: "Nouns are not classified as animate and inanimate, nor are singular and plural distinguished"; in no. 3: "The verbal pronouns do not distinguish animate and inanimate third persons"; in no. 4: "The subject and objective verbal pronouns of the third person do not distinguish between singular and plural"; in no. 10: "In demonstrative and interrogative pronouns, neither animate and inanimate nor singular and plural are distinguished"; in no. 12: "The possessive pronoun of the third person does not distinguish singular and plural." In these five distinct items I discover only two independent statements (one of them, as we shall see below, highly questionable)! When a man pops out the same argument under several disguises, we suspect that he is short of ammunition.

Dr Michelson himself seems to have had a lurking feeling that an abundant use of negative evidence might be dangerous, for he states: "It is perfectly true that many of the above objections are negative, that is, that thus far the phenomena listed have not been reported. It is possible that further investigation may reveal some of them, but it is not likely that a skilled investigator like Dr Kroeber would have overlooked the majority of them." We are all of course delighted to be able to second Dr Michelson's flattering estimate of an esteemed colleague, but when "so careful and able a scholar" as myself is confronted in the wash with the charge of "utter folly," we learn to temper our admiration with prudence. What Dr Kroeber himself states, in a letter recently received from him, in regard to his Yurok material is this: "I have made no serious attempt to analyze the [text] material, contenting myself for the present with pointing out certain features which came to the surface of themselves." In regard to the Wiyot data, Dr Kroeber remarks: "I do not consider the texts very good, nor did I find any satisfactory informant in the time at my disposal. The whole sketch is avowedly a slim preliminary treatment." Under these circumstances it is evident that whoever builds on the non-occurrence of features in Yurok and Wiyot does so at his own risk. As a matter of fact, any one that has had experience in working out in the field the morphology of a language about which absolutely nothing is known to start with realizes that it is perfectly possible to fail to seize many fundamental features for quite a long time. I could give some striking examples from my own experience, did I not fear to lengthen this reply inordinately.

Dr Michelson's use of negative evidence is double. He does not, in the first place, allow adequately for the fragmentary character of our Yurok and Wiyot data. In the second place, it is dangerous to build on negative evidence even if we know for certain that a specific feature is lacking. The history of language shows nothing more clearly than the ease with which grammatical features gradually lose in complexity, often to the point of entire disappearance. What has become of the elaborate Indogermanic case system in French, or of the old complex system of personal endings in English? I maintain that one really striking morphological parallel or half a dozen lexical resemblances buttressed by consistently working phonetic laws (and why does Dr Michelson not occupy himself in his criticism with the phonological material that I have assembled?) are worth a good many points of divergence (of the same "weight").

One negative argument employed by Dr Michelson is so amusing that I must beg leave to comment on it. I had pointed out that Yurok in its adjectives distinguishes between animate and inanimate and drew the obvious parallel with Algonkin, venturing to suggest that other examples of the classification probably would be found in Yurok. Instead of rightly evaluating a striking positive resemblance, Dr Michelson makes capital out of a doubtful negative and cheerfully lists as one of his eight Yurok un-Algonkin features: "Nouns are not classified as animate and inanimate." In other words, "white" is in Yurok associated with animate or inanimate not insofar as whiteness is predicated of an animate or inanimate object but by virtue of some transcendental difference between animate whiteness as such and inanimate whiteness as such. I am afraid that neither the Yurok Indians nor myself feel at home in this highly rarefied philosophic atmosphere.

The one valuable element, I now hasten to add, in Dr Michelson's criticism is his treatment of some of the verbal pronominal suffixes of Wiyot and Yurok that I had compared with Algonkin suffixes. I freely admit that he has made it very plausible that Yurok *-m* 'thou' is not to be compared with Ojibwa *-m(wa)* 'ye,' and that reasonable doubt has been cast on the validity of one or two others of my verbal pronominal parallels. Such corrections must, in the nature of things, be expected and thankfully accepted.

In this reply I have tried merely to point out the serious methodological weakness of Dr Michelson's criticism. No attempt is here made to discuss the evidence for my thesis. For that the reader is referred to the article itself. Let us hope that further Wiyot and Yurok data

will be made available before long, so that more light may be thrown on an interesting and important problem. In concluding I should like to suggest to Dr Michelson that he go through the evidence again in a somewhat more liberal spirit. Perhaps it would be borne in on him that the sum total of lexical and positive morphological resemblances is not so unimpressive after all.

E. SAPIR

*Rejoinder*

It does not seem to me that Dr Sapir has met my point, that it is possible or probable that in his Wiyot, Yurok, and Algonquian word-list, corresponding morphological elements are not being compared. If they are not, the list *per se* has little value.

As to the lack of perspective in linguistic comparison: In the case of Indo-European languages we have historic proof that certain members of the stock have diverged greatly from the original type. The question arises as to whether we have a right to generalize from this, and apply the principle to American Indian languages. I do not think so. As Professor Boas pointed out at the recent meeting of the American Anthropological Association, there are other possibilities to be considered.

If the published accounts of Wiyot and Yurok are merely fragmentary, *per se* there is no more reason to expect that the new evidence will reveal Algonquian traits rather than un-Algonquian ones. But the point I emphasize is that the *existing* evidence does not justify the genetic connection of Wiyot and Yurok with the Algonquian stock. (See page 56 of the *Report of the Secretary of the Smithsonian Institution*, 1914.)

Exactly as Dr Sapir thinks, I over-value the worth of negative evidence, so I think he underestimates it.

Regarding cumulative evidence. I might say that my statement, "Now it is perfectly conceivable that a divergent Algonquian language might possess a few of the un-Algonquian traits mentioned above, but it is incredible that any Algonquian language possesses all of them *en masse*," shows that I have "an inkling" of what it is. Dr Sapir has not answered the point raised in my original article. He lays special emphasis on the Yurok pronouns *n*- 'my', *k*- 'thy', *o*- 'his', *m*- 'somebody's'; and insists that they be treated as  $a+b+c+d$ , and not as  $a, b, c, d$ . Apparently he has not noticed that similar associations occur between Yurok and Wiyot on the one hand and some non-Algonquian languages on the other. For example, Chinook *n*- 'I' (verbal pronoun), Yurok *ne*- 'my'; Chinook *naika*, Yurok *nek* 'I' (independent pronoun); Chinook *m*-, Yurok *-m* 'thou' (verbal pronoun); Yurok and Molala *-k*



'I' (verbal), Yurok *qe-* 'thy', Molala *k'ĩ'* 'thou' (independent pronoun); Karok *na* 'I' (independent), *nĩ-* 'I' (verbal), *ĩm* 'thou' (independent), *mi-* 'thy', *u-* 'he', 'she' (verbal), Yurok *ne-* 'my', *-m* 'thou' (verbal), *o-* 'his, her'; Takelma *gĩ'* 'I' (independent), *ma* 'thou' (independent), Salinan *ke* 'I' (independent), *mo* 'thou' (independent), Costanoan (Monterey) *ka* 'I', 'my', *me* 'thou', 'thy', Yurok *-k* 'I', *-m* 'thou' (both verbal); Shoshoni *nũ-* 'my', *ũm-* 'thy', *u-* 'his', 'her', Yurok *ne-* 'my', *-m* 'thou' (verbal), *o-* 'his', 'her'. So that it does not appear to me that this association works wholly one way.<sup>1</sup>

If Dr Sapir will look over my discussion again he will see that I have not been "discreetly silent" regarding Wiyot and Yurok *m-* 'somebody's'. I pointed out a casual resemblance to Hupa *m-*, admitting that the usage was not the same.

Dr Sapir admits that I have made "it very plausible that Yurok *-m* 'thou' is not to be compared with Ojibwa *-m(wa)* 'ye', and that reasonable doubt has been cast on the validity of one or two others of my [Dr Sapir's] verbal pronominal parallels." It certainly would be of scientific interest for him to have stated just which these are. For my own part I think I have not only refuted the *m* comparison, but four others as well; "cast reasonable doubt" on that of *wa* of Wiyot *kiluwa* 'you' with Ojibwa *ki-wa* 'your'; and to have shown that the comparison of Wiyot *hu-* with Fox *u-* may not be so certain after all, as Wiyot *hu-* occurs in the first person plural as well as in the third person. I regret that Dr Sapir has not seen fit to combat my reasoning in any of these cases. I have not "sedulously avoided" challenging certain "really convincing *positive* points" in his paper unless he acknowledges the pronouns under discussion are not convincing.

I have examined Dr Sapir's word-list again, and am even farther from being convinced by it than when I first read it. Dr Sapir has noted that several Algonquian secondary stems seem to be cognate with Wiyot primary stems. He cites Ojibwa *nin gābaw* 'I stand' (from Baraga) to show that even Algonquian dialects may differ from one another in this respect. I can not confirm this: in all Ojibwa dialects of which I have any independent knowledge, *gābaw* can not occur initially. That Dr Sapir can not be held responsible for the error, I freely grant; but the proposition to equate primary with secondary stems does not seem to me to be legitimate.

It should be noted that the comparison of Wiyot *me-weril* 'flesh', 'fat' with Ojibwa *winin* 'fat', Natick *weēs*, *weis* 'fat', Micmac *weoo's*:

<sup>1</sup> I take the opportunity to state that my comparison of Miwok *-tok*, Wiyot *-itak*, is a mistaken one.

'flesh', Cree *wiyās* 'flesh', can not stand. The ordinary phonetic laws bar the comparison of all the Algonquian words: the Ojibwa word can have nothing to do with the others; cf. also Kickapoo *wīnenwi* 'fat', *wīyādi* 'flesh'. Similarly Fox *uwīw(i)* 'to marry' and Ojibwa *widige* 'to be married' cannot be associated (see *Jour. Washington Acad. Sciences*, IV, p. 404). For a similar error see the comparisons under Wiyot *wet* 'satisfied'. There are some Wiyot-Yurok comparisons, such as Wiyot *hekʷ* 'snow' with Cheyenne *hístās* 'snow', Yurok *rokʷ* 'wind' with Ojibwa *nōdin* 'wind', Cree *yōtin* 'wind blows', and Wiyot *wayit* 'to bend' with Ojibwa *wāgina* 'to bend', Cree *wākisiw* 'he is bent', which do not conform to the phonetic laws laid down by Dr Sapir. Certain Wiyot comparisons involve the assumption of prefixes in the nouns of that language, against which see Kroeber in *University of California Publications in American Archaeology and Ethnology*, vol. IX. Under the circumstances it would have been better to leave out such comparisons. Wiyot *tem* 'to sit' is compared with Delaware *lúm<sup>a</sup>i-api-* 'to be seated'. I can not analyze the first part of the latter term, but the last part suggests comparison with Fox, etc., *api-* 'be seated'. Though perhaps not germane to the subject, I should like to point out that Cree *-n* of the first and second persons singular indicative is not identical with the *-n* of the third person inanimate indicative of intransitive verbs: in Fox in the first case there is no correspondent, e. g. *-si*; in the second *-enw<sup>i</sup>*, in which *-en-* is a secondary connective stem, and *-w<sup>i</sup>* the inanimate termination lost phonetically in Cree. I have not listed here all comparisons to which exception might be taken; but I may add the number of positive errors that *at present* can be demonstrated as such, is below the fifty percent allowed by Dr Sapir.

A word on Dr Sapir's criticism of my list of the un-Algonquian morphological features of Wiyot. He has suppressed the second part of No. 3—"nor are the exclusive and inclusive first persons plural distinguished." So there are more than "only two independent statements" in Nos. 2, 3, 4, 10, 12; nor is "one of them" "highly questionable." Dr Kroeber has not pointed out that in *Wiyot* animate and inanimate third persons are distinguished, even if he has pointed out that in *Yurok* certain adjectives apparently do make such a distinction. *Yurok* and *Wiyot* are not the same. That Dr Sapir's remarks apply to the *Wiyot* and not the *Yurok* list is shown by the numbers attached to the quotations. As to the distinction made in *Yurok* regarding adjectives, I think there is need of further material before this point can be elucidated.

As to the point raised by Dr Sapir with respect to vocalic change in participials, it is sufficient to refer to what I have said, namely, "Now it

is perfectly conceivable that a divergent Algonquian language might possess a few of the un-Algonquian traits mentioned above." I am by no means satisfied that vocalic change in the participial is lacking in Natick, though I have not sufficient material at hand to demonstrate it absolutely. Lest the reader think from Dr Sapir's language that the change is confined to Fox, Cree, and Ojibwa, I may assure him that it is found in several other Algonquian dialects.

If most of the list of un-Algonquian traits of Wiyot are "trivial," it surely would have been easy for Dr Sapir to refute them one by one, which he has not done.

At the time at my disposal (I saw Dr Sapir's article in galley proof only) it is not possible for me to make so long a list of words that resemble one another in Algonquian and in other languages than Yurok and Wiyot as Dr Sapir has made of Yurok, Wiyot, and Algonquian; at the same time the following will indicate that such a list probably could be made with sufficient time, even if an "onerous task."

YUKI	ALGONQUIAN
<i>kan</i> , talk	Fox, <i>kan-</i> , <i>kanō-</i> , talk
<i>mi</i> , drink	Fox, <i>meno-</i> , drink
<i>muy</i> , futuere	Fox, <i>ma</i> , futuere
<i>na<sup>w</sup></i> , see	Fox, <i>nāw-</i> (really <i>nā-</i> ), see
<i>pan</i> , fall, stumble	Fox, <i>pāna-</i> , fail, miss <sup>1</sup>
<i>tas</i> , snare, trap	Fox, <i>tes-</i> , snare, trap
<i>pok</i> , burst	Fox, <i>pō'k-</i> , break
<i>tuk</i> , strike	Cree, <i>takiskawew</i> , strikes him with the foot
YOKUTS	ALGONQUIAN
<i>na</i> , I (verbal)	Fox, <i>ne-</i> (verbal), I
<i>nān</i> , we (pl. excl.)	Cree (Fort Totten), <i>-nān</i> , we (excl.)
<i>yūwin</i> , wife	Fox, <i>uwūwani</i> , his wife
<i>ponpon</i> , snow	Fox, <i>pepōn<sup>i</sup></i> , winter
<i>met'</i> , large	Ojibwa, <i>mitchā</i> , it is large
<i>bok</i> , find	Fox, <i>me'k-</i> , find
<i>padu</i> , enter	Fox, <i>pīti-</i> , enter
<i>wid</i> , say, tell	Fox, <i>wīl-</i> , tell
<i>dapi</i> , pick, gather	Fox, ( <i>A</i> ) <i>tāp-</i> , pick up

I maintain that these random Yuki and Yokuts words resemble Algonquian ones fully as closely as do the most striking Yurok and Wiyot words of Dr Sapir's list. Would Dr Sapir conclude from the Yuki and

<sup>1</sup>Cf. *pānatakanī*, 'you must have let it fall astray,' Jones, 230.11.

Yokuts list that Yuki and Yokuts are Algonquian languages, even if their morphology is fundamentally un-Algonquian, so long as we have a trifle more than "half a dozen lexical resemblances" and "one really striking morphological parallel" in that Yokuts and Algonquian distinguish the exclusive and inclusive first person plural?

TRUMAN MICHELSON

### *Epilogue*

Dr Michelson evidently dies hard. His "rejoinder" to my reply contains quite a number of statements that I would have something to say about, did I not feel convinced that the reader is as sick of this fruitless squabble as I am. I shall therefore let the real reply to both Dr Michelson's "rejoinder" and original criticism consist of a reference to my article on "Wiyot and Yurok, Algonkin Languages of California." I prefer to have the reader form his opinion of the merits of the case from the article itself than from either the article as interpreted by Dr Michelson or as explained by myself. For further criticism, I shall confine myself to expressing regret at the second paragraph of Dr Michelson's "rejoinder," as it makes capital of certain opinions which (whether justifiable in themselves or not is another matter) are not at hand for the reader to refer to.

E. SAPIR

### A NOTE ON KINSHIP TERMS COMPOUNDED WITH THE POSTFIX 'E IN THE HANO DIALECT OF TEWA

In discussing the kinship terms of the New Mexican Tewa, Mr J. P. Harrington and Dr E. Sapir<sup>1</sup> have called attention to the formation of reciprocal terms by adding the suffix 'e (which is ordinarily postfixed to nouns to form diminutives such as *musa'e*, 'little cat,' *kege'e*, 'little house,' and also used as an independent noun meaning 'child,' 'offspring') to express the relation of junior to senior relatives. In the Hano dialect<sup>2</sup> there are nine such pairs of terms:

SENIOR	JUNIOR
I. <i>Məməx̄</i> , mother's brother, etc.	<i>məməx̄'e</i> , man's sister's child.
II. <i>Ko'o</i> , mother's sister, etc.	<i>ko'o'e</i> , woman's sister's child.
III. <i>Ka'je</i> , mother's elder sister, etc.	<i>ka'je'e</i> , woman's younger sister's child.

<sup>1</sup> J. P. Harrington, Tewa Relationship Terms, *Amer. Anthr.*, N.S., 14, 1912, 472-498; E. Sapir, A Note on Reciprocal Terms of Relationship in America, *Amer. Anthr.*, N.S., 15, 1913, 132-138.

<sup>2</sup> See B. Freire-Marreco, Tewa Kinship Terms from the Pueblo of Hano, Arizona, *Amer. Anthr.*, N.S., 16, 1914, 269-287.